to say quantitatively that such a metal or alloy will be ferromagnetic and such another one not." This is not surprising, since Prof. Fowler admits that the most the *R-B* atom can do is to suggest that the conditions for ferromagnetism "might" be most easily fulfilled among the metals and alloys of the iron group. This is a long way from explaining, as the *A* atom does in its stride, why ferromagnetism is sometimes *absent* in alloys of the iron group (manganese steel), and sometimes *present* in alloys not of the iron group (Heusler alloys).

(3) *Spectroscopy.* Prof. Fowler must have overlooked p. 39 of my book which begins: "*Under suitable conditions,* every element will yield an emission spectrum . . ." On the other hand, Prof. Fowler himself refers to the oxygen atom as "stubborn". It is this spectroscopic property of oxygen, and of other elements, qualitative, but none the less real and important, which is predicted by the *A* atom.

(4) *Chemistry.* "In the chemical field, the qualitative successes of quantum mechanics and the *R-B* atom seem even more striking." Now, if there is one fact which more than any other is fundamental in chemistry and metallurgy, it is the distinction between metals and non-metals, and since the *R-B* atom cannot make this distinction, except for the alkalis, it is doubtful whether chemists and metallurgists share Prof. Fowler's enthusiasm for the striking successes of the *R-B* atom in this field.

(5) *Alpha Scattering.* "When it [an alpha particle] passes near one of the protonic complexes it will also be scattered by Rutherford's law with a factor P^2 when the charge on the complex is Pe." This criticism, if valid, would effectively dispose of the *A* atom, but it is invalid, because the protonic complexes are not rigidly *fixed* as are the atomic nuclei, but move in quantised orbits, and the recoil conditions are totally different. Since the Rutherford law cannot be applied in the way suggested by Prof. Fowler, he is scarcely justified in basing thereon the claim that: "The alternative atom fails outright, self-strangled at birth."

Nevertheless, in regard to the dimensions of quantised orbits of heavy particles, I admit that the calculations are so difficult that I have not been able to make them, but that does not prove that the *A* theory is wrong, nor can it yet be assumed that the *A* theory is incompatible with Coulombian binding forces, particularly since, as Prof. Fowler admits, it is doubtful whether the scattering experiments are able to test this rather fine distinction.

(6) *Isotopes.* This question does not permit at present either of proof or disproof of the *A* theory, but it does open up certain new lines of research which seem worth pursuing.

(7) *Collision Theory.* Finally, Prof. Fowler reproaches me for not touching on this subject. I remedy this omission, since it throws some light on the question at issue, namely, whether the outer regions of atoms are always negatively charged, as for the *R-B* atoms, or whether, as in the case of the *A* atom, they are positively charged for atoms such as oxygen, and negatively charged for atoms such as argon, or positive *cum* negative for a molecule such as ethylene. Here is something which can at once be tested by mixing these gases under pressure, to see whether there is any tendency towards cohesion. This has already been done by Prof. Irvine Masson, who has observed an abnormal cohesion on mixing oxygen and argon, oxygen and ethylene, and argon and ethylene[1]. This curious phenomenon, which is precisely what one would expect on the *A* theory, has never been explained in terms of the *R-B* theory.

Thus, in spite of the criticism which Prof. Fowler has directed against the new theory, I hope that it will receive further serious consideration and discussion.

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[1] Masson and Dolley, *Roy. Soc. Proc.*, A, **103**, 524; 1923.

I SHOULD have been much disappointed if Dr. Tutin had taken my review of his book 'lying down'. Indeed he has not done so, but he has added little or nothing in his reply to his development of the *A* atom. Paragraphs (1)–(4) merely reiterate what he claims that the *A* atom has done and the *R-B* atom has not. These claims are frankly preposterous.

In my reasoned criticism of Dr. Tutin's book, I took some pains to state at length the present position of the quantum mechanical theory of matter based on the *R-B* atom, and great care to avoid any overstatement of its successes. When it was said that this theory leads to such and such definite results, it was implied that these results were logical consequences of the theory, based on its initial postulates and without any *ad hoc* hypotheses whatever. Chapter and verse can be given for the proofs of all such results. Where I expressed the opinion (a pious hope!) that such and such phenomena were probably explicable in the same way, it was again implied that they were probably thus explicable without additional hypotheses, certainty being merely held up by mathematical complexity, a very different thing from difficulties in or uncertainty of physical principles. In contrast to this, there is not one single statement of a result in the whole of Dr. Tutin's book which can be regarded as there presented as a logical deduction from a definitely stated theory; the scattering laws should perhaps be excepted and to these I return later. I do not assert that his results can never be so presented. I maintain only that none of his results has yet been so presented and that most of them never will be. His candid admission in the last part of section (5) of his reply shows this so clearly that comment is scarcely necessary. It is no use just repeating the claim of what the *A* atom can do, in magnetism, for example. What is required is logical deduction.

I return now to the scattering of α-particles, not because anything need be added to, or withdrawn from, my former criticism, in order to meet Dr. Tutin's reply, but because it is perhaps worth while analysing this reply as a typical example of the vague and unsatisfactory nature of Dr. Tutin's reasoning throughout his book. Briefly, the gist of his objection here is as follows. He has two force centres, *A* and *B*, bound together by certain forces, and a third body *C* collides with them. When *C* goes close to *A*, *A* and *B* are effectively rigidly connected and recoil as a whole. When *C* goes close to *B*, this does *not* happen. Yet the particles are said to obey the laws of quantum mechanics! In any mechanics except the Tutinian, what is sauce for the goose is sauce for the gander, and I cannot withdraw my verdict of self-strangulation, which Dr. Tutin would apparently accept apart from this plea of non-reciprocity.

In the last paragraph of his reply, Dr. Tutin directs attention to some interesting experiments by Prof. Masson on the equilibrium p-v isotherms of binary mixtures of oxygen, ethylene and argon. These experiments on the *equilibrium* states of gas mixtures have little or nothing to do with collision theory as usually understood; this, however, is unimportant. Dr. Tutin claims that the evidence they supply for a fairly strong attraction between pairs of molecules of these gases is in favour of his theory and against the *R-B* atom, because with the *R-B* atom all these molecules will be negatively charged on the periphery. He overlooks entirely the fact that for electrically neutral systems (such as these molecules) the residual polarisation effects with which we are here concerned always yield an extra attraction no matter what the unperturbed arrangement of their charges. This is a classical result which prevents discrimination between such theories in any such way.

Here, for the present at any rate, one may well take leave of Dr. Tutin's theory. *If* it ultimately proves of value and supersedes the *R-B* atom, no one will be more surprised than the present reviewer —and no one more delighted. Such success would imply the construction of a logical theory appreciably more successful than current theory in interpreting the properties of matter, and current theory is a lusty infant of whom its parents and guardians, even metallurgical and chemical, are justifiably proud.

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Spontaneous Emission of Neutrons by Artificially Produced Radioactive Bodies

I. CURIE, F. Joliot and P. Preiswerk[1] observed that after bombarding silicon or phosphorus by neutrons, the artificially produced radioactive nuclei emitted: (1) negative electrons as previously observed by Fermi[2]; (2) γ-rays of high energy ($\sim 5 \times 10^6$ e.v.); (3) positrons (which they tentatively suggest to be due to the creation of 'pairs' by the γ-rays, an explanation which does not seem very probable as they state that the positrons have an upper energy limit of $\sim 1 \times 10^6$ e.v. only); and (4) *neutrons*. They assume that some of the radioactive nuclei are $_{13}Al^{28}$ and $_{14}Si^{31}$, and that the spontaneously emitted neutrons are due to the transformation processes:

$$_{13}Al^{28} \rightarrow {}_{13}Al^{27} + {}_0n^1 \text{ and } {}_{14}Si^{31} \rightarrow {}_{14}Si^{30} + {}_0n^1.$$

If this formal explanation is correct, the neutron must have existed in its parent nucleus in a state of *positive* energy. But this would mean that there exists in a nucleus a potential barrier for neutrons, which would not only contradict current theoretical views about the interaction forces between neutrons and neutrons or protons[3] but would also be in disagreement with Fermi's[2,4] discovery that neutrons easily penetrate into nuclei of all charges and masses.

The spontaneously emitted neutrons must therefore get into a state of positive energy by some primary radioactive process, and then be *immediately* emitted. Assuming the potential energy of a neutron in the field of any nucleus to be everywhere negative, and the potential energy of a proton to show a 'barrier', we have two possibilities for explaining the emission of neutrons:

(i) In the radioactive nucleus A, all negative energy states of protons are occupied and there exists *one* proton in a state of positive energy. This proton may either penetrate the barrier of A or transform into a neutron, emitting a positive electron. We must assume that the neutron can be created in a state of positive energy. It can then *either* be emitted *or* fall into a state of negative energy, causing an emission of a γ-ray. But it is rather unlikely that an unstable nucleus produced out of a stable nucleus by neutron bombardment (and therefore short of protons) should not have an unoccupied negative energy state for a proton. If A has an unoccupied negative energy state, a proton cannot remain an appreciable time in a state of positive energy.

(ii) The radioactive nucleus A contains two loosely bound neutrons. One of the neutrons (n_1) transforms into a proton, emitting a negative electron. The proton may be created in an excited state p' and then fall down to a lower state p, whereby it can emit a γ-ray. Alternatively, the transition energy can be handed over to the second neutron (n_2) which will thus be raised to a state of positive energy and will then be emitted. This model seems preferable to (i).

Recent observations of M. Mäder[5] seem to show that samarium emits protons spontaneously. As samarium is known to emit α-rays[6], it may be that the emission of a proton (or alternatively the emission of a γ-ray) follows immediately after the emission of an α-ray. If this is not the case, we must conclude that one isotope of samarium contains a proton in a positive energy state. From (i) we might then expect samarium also to emit positrons, for a nucleus with a proton in a positive energy state certainly has a lower unoccupied state for a neutron, which makes a proton → neutron transition energetically possible. The neutrons can then only be emitted if they are created in states of positive energy.

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[1] *C.R.*, **198**, 2089, June 11, 1934.
[2] *Ricerca Scientifica*, V, **1**, 283, 330; 1934. NATURE, **133**, 757, May 19, 1934.
[3] W. Heisenberg, *Z. Phys.*, **77**, 1; 1932. **78**, 156. **80**, 587; 1933. E. Majorana, *Z. Phys.*, **82**, 137; 1933.
[4] NATURE, **133**, 898, June 16, 1934. E. Amaldi, O. D'Agostino, E. Fermi, F. Rasetti, E. Segre, *Ricerca Scientifica*, V, **1**, 452; 1934.
[5] *Z. Phys.*, **88**, 601; 1934.
[6] G. Hevesy and M. Pahl, NATURE, **130**, 846, Dec. 3, 1932.

Hyperfine Structure of the Resonance Lines of Potassium

THE hyperfine structure of the resonance lines (7699 and 7665 A.) of potassium has been investigated by means of absorption in a potassium atomic ray. The lines were obtained in emission from a discharge tube containing neon at a pressure of a few millimetres, and potassium vapour at a pressure of less than one two thousandth of a millimetre; the tube was excited by means of external electrodes, and was of the type used by Jackson in previous investigations on the structures of resonance lines.

Before entering the spectrograph, the resonance light passed through a ray of potassium atoms, the direction of the atomic ray being at right angles to the line of sight. The atoms forming the ray passed through a cool tube the length of which was twenty times greater than the width, so that the component